



A
VINDICATION
OF THE
NEW METHOD
OF
INOCULATING
THE
SMALL-POX, &c.



[Price Two Shillings.]

WINDY DAY

NEW METHOD

INOCULATING

SMALL-POX

[Price Two Shillings]

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A
VINDICATION
OF THE
NEW METHOD
OF
INOCULATING
THE
SMALL-POX,

AGAINST THE
ARGUMENTS and OBJECTIONS
OF
DR. LANGTON and MR. BROMFEILD,

Wherein it is demonstrated,

That the Distemper communicated by this Method
is the **GENUINE SMALL-POX;**

That Patients so inoculated are not in the least Danger
of taking the Distemper in the **NATURAL WAY;**
AND ALSO

That the Method itself bids fair, in the Hands of judicious Practitioners, to become in the highest Degree beneficial to Mankind:

In which is comprehended

An **INQUIRY** into the **TRUE CAUSES,**
Which render the Distemper so very favourable,
In **THIS Way of INOCULATING.**

By **GILES WATTS, M. D.**

Opinionum commenta delet dies ; Naturæ judicia confirmat.

Cic. de Natur. Deor. Lib. II. § 2.

L O N D O N:

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MDCCLXVII.



INTRODUCTION.

THE various and manifold necessities and dangers, to which mankind are continually exposed in this imperfect state of sublunary things, excited men, in the earliest ages of the world, to exert their utmost abilities in search of means, whereby to render their situation in this life more comfortable and secure. To these efforts we owe the numerous useful inventions, that have been delivered down to us from antiquity. Notwithstanding, however, the vast variety of inventions conducive to the comforts and conveniencies of life, which were effected by the ancients, they still left a multitude of others to exercise the ingenuity of those who succeeded them; an observation, which has

been abundantly verified by the various ingenious discoveries, that have been made in the arts and sciences within these few last centuries.

Ingenious, however, and useful as many of these discoveries have confessedly been to society, one common misfortune seems, in a great measure, to have attended them all. For it is but too true, however melancholy a consideration it may be, that there are few, very few indeed, of them, which have not met with an almost infinite deal of opposition; which have not been obliged, as it were, to wade through an ocean of prejudice, before they have been established on a solid foundation. This was, to confine myself to matters of medicine, in a most remarkable manner, the lot of the very useful discovery of the circulation of the blood made by the immortal Harvey: and this too has already been remarkably the lot of that other very useful discovery, inoculation of the small-pox: discoveries, that have, doubtless, been the means of saving multitudes of lives, and whose value, if measured by the greatness of their utility to mankind, most certainly surpasses that of any two discoveries, which the ingenuity

ingenuity of man was ever yet capable of effecting. The one has been the means of pointing out to us the nature, and most likely method of cure, of a vast variety of dangerous disorders; the other of rescuing us, as it were, from the jaws of one of the most dreadful and merciless distempers incident to mankind. The first, indeed, of these discoveries has long ago surmounted all opposition; and the last had well nigh done the same, at least, in this island. But now that a most extraordinary improvement is made in it, and the art of inoculation is enabled to reduce the distemper to almost as low a degree as we could wish, the old objections, which were long ago refuted, are revived, and that with more vehemence than ever, as if the nearer the art is brought to perfection, and the more likely it is to become beneficial and salutary to society, the more difficulties and obstacles were to be thrown in its way. It was formerly thought by sensible and considerate people, an advantage great enough on the side of inoculation to encourage a general use of it, that scarce one in an hundred died under the operation. It now appears from the incontestable evidence of experience, that

of late not a single patient in twenty times that number has been lost. And I will be bold to say, that whoever has been conversant amongst patients inoculated in the old way and in the new, must with the utmost imaginable obstinacy resist the evidence of his own senses, or be thoroughly convinced of the great excellence of the last method above the first. And if he is one too, who has the welfare of mankind in any wise at heart, he must be not a little concerned to see it meet with so much opposition, as it has lately met with. This is the very case of the author. He entertains through experience an high opinion of the present method of inoculation, and is really sorry to see it condemned in so positive and absolute a manner, as it has been of late. A few years ago, he had two of his sons inoculated by a very judicious and experienced practitioner, in the old way. One of them had the confluent small-pox, and hardly, very hardly, escaped with life; and the other too had the distemper very severely. He has often visited patients under inoculation in the old way. And he does not remember, that he ever knew a company of ten or a dozen inoculated together in that way,

way, but one, or more of the company has had the distemper in a pretty severe manner. Very lately he has had four of his family inoculated in the new way, and all of them together have not had so many as eighty pustules. He has not only been conversant amongst patients inoculated in this way by other practitioners, but has also within these few months been concerned in the inoculation of many hundred persons himself, and that without having had the misfortune of losing a single patient. These occurrences have given him the fairest opportunity of judging between the two methods; and he is thoroughly convinced, that the superiority is almost infinitely on the side of the new. But this is not all. He has been called to several patients, who have been inoculated in the new way by different practitioners, some of whom have been of the first class. A circumstance, which has afforded him an opportunity, not only of seeing what a very small number out of the multitudes of persons of all ages, habits, and constitutions, who have been inoculated lately in these parts, have been ill after it;* but likewise of discovering to what peculiar medical treatment,

* Suffex and Kent.

or what particular mismanagement in the patients, those disorders have generally been owing. And he cannot say, but he firmly believes, the disorders consequent on this method of inoculation, have oftner taken their rise from the errors of the patients themselves, than from those of their inoculators. Nor has he, so far as he can remember, been called to a single patient, whose death can be fairly placed to the account of the effects of inoculation. From all these circumstances taken together, the author acknowledges, he has conceived a most high opinion in favour of the present method of inoculation. And he is sorry, extremely sorry, to see a practice, which, he is persuaded, is well calculated to answer the purpose of saving numbers of useful lives from the ravages of a cruel distemper, depreciated, especially by those, whom it would have much better become, their profession considered, to have given it all the encouragement they were able. At the same time he cannot but confess, that, when Mr. Sutton first came into Kent to inoculate, and such strange accounts were almost daily received of the surprising success of his practice, as were enough to stagger the faith of
any

any man, he himself, amongst others, was much inclined to suppose there was some deception or other in the matter. However, as he had, for many years, entertained an opinion, that it was in the power of art to render the small-pox much lighter on patients, than it generally proved in the old way of inoculation, and that the want of success, in that method, had been ever principally owing to the circumstance of inoculators neglecting to pursue a diet and regimen sufficiently cooling, he was willing to suspend his opinion of the matter, till he could have an opportunity of seeing, himself, some of Mr. Sutton's patients actually under the distemper. Several such opportunities soon offered. And the author had many times ocular demonstration, not only that the distemper was extremely light on Mr. Sutton's patients, but also that it was the *true genuine* small-pox. He was now no longer surpris'd at the many accounts he had before heard of this gentleman's great success. For he imagin'd, he could plainly see, through an acquaintance with his management of his patients, the true causes, to which principally this success of his was owing. And he is still of the
same

same opinion in this respect, which he was of then. Nor can he forbear expressing his sorrow at finding, that any physician can entertain so low an opinion of the power and efficacy of the art he professeth, as to think the distemper communicated in the new way of inoculation, cannot be the small-pox, merely because patients so inoculated contract only a slight disorder. It was early, therefore, that the author conceived a favourable opinion of the new method of inoculation. How this favourable opinion of his arrived to its present height, he has already shewn. He has therefore nothing farther to offer by way of apology for his present undertaking, or of introduction to the subject matter of this treatise, than that his sole motive for taking pen in hand, on the present occasion, was a desire of vindicating a practice, which he looks upon as likely to prove highly salutary to mankind, from the unjust imputations, which have been lately thrown upon it, and of setting it in its true light. And if he shall succeed in his design, he shall be satisfied, he has rendered a very essential piece of service to his fellow creatures; a reflection, which will abundantly pay him for his pains.

A V I N-



A

VINDICATION, &c.

IN vindicating the new method of inoculation, I think it necessary to begin with considering the arguments and objections of Dr. Langton, because, if what the Doctor has been pleased to affirm in the title page of the pamphlet he has lately published on the subject, be matter of fact, all argumentation in favour of this method must be entirely useless, and the practice, as it is now followed, must necessarily fall to the ground. But unluckily for himself, he has there asserted a thing which is diametrically contrary to the experience of thousands, and which neither he, nor any other person on the face of the earth, is, or ever will be, able to prove. He there positively asserts, that *the matter communicated* in the present method of inoculation *is not the small-pox*, and through the whole almost of his pamphlet he has strained

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every

every nerve in proof of this assertion. What can have been the Doctor's motive for this attack of inoculation he himself best knows. If one were to judge of it by the vehemence and rancour with which he has inveighed against both the method and its practitioners one would be inclined to suspect it to be a motive not of the most commendable kind. In his preface indeed he has declared, that *neither the desire of appearing in print, nor the affectation of being particular, much less any intention of injuring the practice of inoculation, induced him to compose his pamphlet*: and he has added, that *principles of the most benevolent kind, and such as concern the lives and happiness of his fellow subjects, were the sole motives to his undertaking*. I shall not presume at present to call in question the truth of any thing the Doctor here asserts. Thus much, however, I cannot forbear observing on the occasion, that the expression *gross imposition*, which is applied to the present method of inoculation more than once in his pamphlet, and some other passages occurring therein, seem to imply no inconsiderable share of prejudice in the author, and that it is a pity, for his own sake, he did not inquire more carefully into the success and nature of the practice, that he might have urged other than mere speculative and theoretical arguments against it, before he condemned it in so positive and so absolute a manner as he has done; for, to use the words of a very ingenious and learned, as well as

a very candid * writer on the subject, *he is an enemy to improvement, and is no philosopher, who fastidiously, and upon mere speculation, rejects what he has not brought to the test of experience.* And I will add too, he is not serving the cause of truth and the publick, who endeavours to depreciate and discountenance a practice likely to prove highly beneficial to society, without giving himself time to examine properly into the merits of it. If the Doctor had done this on the present occasion, sure I am he had never ventured to publish many things, which he has advanced in his pamphlet, against the present method of inoculating the small-pox, as I shall now proceed to demonstrate.

The first objection of the Doctor's against this method, which I shall take notice of, is one deducted from a notion, that *Mr. Sutton, in the three last years of his practice has made forty or fifty thousand pounds.* How far this notion may be true, it is altogether out of my power to determine. But suppose Mr. Sutton has gained by his practice twice, or even ten times as much in the time specified, would this circumstance be any proof of the inutility of inoculation? Certainly no man can say it would. And I must needs say, it shews too evident a want of candour in the Doctor to urge it as such on the occasion. If such a

* Vid. Dr. Baker's Inquiry, &c. Page. 4.

circumstance can be of any moment in the case, it must, most certainly, tend to shew the direct reverse of what the doctor has adduced it in proof of, namely, that the practice is in itself good, and has answered its professed design in securing patients from the contagion of the natural small pox. Had not this been the case, out of the many thousands, whom Mr. Sutton has inoculated, some few at least must, inevitably, have taken the natural small pox before now, and his practice have consequently and necessarily fallen into the utmost discredit. But I believe I may defy the Doctor to produce a single instance of such an accident. Indeed to say the truth, had he known of any, no doubt is to be made, but he had long ago published it to the world.

In the same passage, in which the above objection occurs, the Doctor has insinuated in order to discredit inoculation, that it is **the interest of inoculators, that propagates and continues the practice.* Alas! Must then the very profits, arising to the practitioner from the exercise of the art he professeth, be urged as a proof of its inutility? This is very unfair indeed. At this rate every art, which brings reasonable gain to him, who practises it, must be condemned as useless, or, what is worse, as a piece of *gross imposition.* This Doctrine is

* Vid. Dr. Langton's Address, &c. Preface P. 2.

downright uncanonical. *The Labourer*, surely, is worthy of his hire. If this will go down, then it is high time for us to cry out, *miserere nostrum Domine*, for all of us, clergymen, lawyers, physicians, surgeons, &c. &c. &c. must be of necessity involved in the same common danger—of starving. To be serious, the Doctor is, it seems, most violently prejudiced against this same inoculation, and something or other must be urged against it at all events. It matters not much with him, how little to the purpose.

The Doctor next takes notice of an advertisement of two gentlemen inoculators, residing, I suppose, somewhere in his neighbourhood. It must be confessed, the advertisement is far from being drawn up with the greatest precision and accuracy. It is, however, hardly fair in him to make the use of it he has made in his pamphlet. The gentlemen, I presume, intended only to inform the publick, *that the small pox communicated in the new way of inoculation is generally so very light, that the patient is sometimes apt to imagine there is danger of a second infection; that, for the satisfaction of patients in such cases, they have inoculated them again and again; and that if ever the natural small pox has been taken after inoculation, it must have been owing to the fault of the inoculator.* All this is very true and very reasonable. But they do not say a word, that a second infection ever happened to

any one of their patients, even by inoculation, much less by natural contagion. And yet the Doctor adduces this advertisement as a proof, that in this way a * *second infection is to be dreaded*, and that *second infections are no unusual accidents*. I must here beg leave to inform the Doctor of a few circumstances, with which he seems totally unacquainted. If this had been the case, he could not have made so unfair a use of the above advertisement, as he manifestly has. In the old way of inoculation, it was the custom of the inoculators to make a small incision in the arm of the patient, and to insert into it a pellet of lint or cotton moistened with variolous matter, which pellet was covered with a plaister, and kept on for a longer or a shorter time, according to the opinion the operator happened to entertain of the time necessary for it to lie on in order to infect the patient. This was the general method, in which the operation was preformed, and it is a fact well known to those concerned in it, that the operation pretty often miscarried, and the patient was forced to be inoculated again and again, before he could be infected. Nor have there been wanting cases, wherein the patient has not been infected before the fourth or fifth time of its being performed. I myself have known eight patients inoculated in this way, at the same time, and in the same

* Vid. Dr. Langton's Address, &c. Preface P. 5 and 6.

house,

house, and by the application of the same variorious matter, and the operation has taken place in one half, and miscarried in the other. Sometimes, I suppose, the operation has failed to infect the patient, because the matter had been kept too long, or in too cold a place, or by some such circumstance had lost its virtue. At other times, I suppose, the current of blood flowing from the wound has washed away the infectious miasmata applied to it, and so prevented their being imbibed into the mass of humors, and consequently the patient from being infected. From what cause soever it has happened, it is, however, well known that this method of inoculating has often miscarried. And on the contrary it is a fact equally as well known amongst inoculators in the new way, that this seldom or never fails of infecting. I myself have been concerned lately in the inoculation of many hundred patients in this new way, and out of upwards of twelve hundred punctures, I have not known a single one miscarry. Nor do I make the least doubt, but many other inoculators in this way, have experienced the same success. It may not perhaps be unnecessary here to inquire a little minutely to what particular circumstances attending this new method, this its superior success in this respect is owing.

In the first place, I am of opinion, that the taking the variolous matter, and inserting it into the patient, while it is yet warm, contributes not a little to ensure its succeeding. And in the next place, I suppose, that the infusing it the very moment, when, and on the point of the same instrument, with which, the wound is made, is another circumstance, that contributes equally as much to effect the same purpose. From these circumstances it happens, that the variolous matter is applied in its full efficacy and vigour, to the mouths of the wounded vessels of the skin, and therefore can hardly fail of infecting the patient. We see that this method of communicating the virus of the small pox is very similar to that, by which many venomous animals are wont to communicate theirs. To wit, in both cases, the wound is made and the virus infused warm into it, at the same moment, and therefore it cannot possibly fail of infecting. And I have several times known the operation succeed in infecting a number of patients together without a single failure, when there has been so little variolous matter in the pustules on the arm of the patient, from whom they were inoculated, that nothing, but a little bloody sanies, could be discerned on the point of the lancet, and the operator himself could scarce believe it could possibly succeed. Of such extreme consequence is it towards ensuring an infection of the patient to in-
fuse

fuse the variolous virus, while it is yet warm, into the blood. From these considerations it is very evident, how greatly superior, in point of certainty of infecting, the method of operating in the present way is to that used in the old.

Having now sufficiently, I hope, demonstrated, that the new method of operating seldom or never fails of communicating the small pox, I am next to enquire, to what causes it is owing, that a repetition of the operation, when inoculation is performed in this way, is sometimes necessary.

It is a fact sufficiently notorious to those, who have been much conversant amongst patients under the natural small pox, that some of them, especially children, have the distemper extremely lightly. I have seen a patient in this way, who has been altogether unprepared, and yet has not had more than four or five pustules, and those have been but very small ones. Nay, it is an observation of Doctor * Boerhaave, a writer, whose authority Doctor Langton himself has cited, that the *small pox often happens without any pustules at all*. It cannot, therefore, be matter of surprise, surely, to any reasonable practitioner, if the same circumstance should every now and then happen

* Vid. Boerhaav. Aphorism. § 1393. *Morbus variolosus saepe sine variolis fit.*

to a patient under inoculation, especially in the new way. To say the truth, it is a fact well known to inoculators in this way, and I have sometimes known the same happen in the old, that the patients pretty often pass through the small pox so easily, as to have no more than five pustules. Nay, it happens every now and then in this way of inoculation, that even an adult patient shall pass through the distemper without having one, or even so much as a single complaint, other than, perhaps, a slight shivering, chill, or some such trifling disorder, which he would hardly have taken the least notice of at any other time. So very powerful is the present method of preparation, and management, in lessening the violence of the distemper. When this happens, and especially if it happens without a considerable inflammation of the skin round the punctures, the patient can hardly be brought to believe, he has had the small pox. In such cases therefore, it is ever prudent in the operator, for the satisfaction of the patients, to inoculate them again. And it was on such occasions, that the above mentioned inoculators observe, they have inoculated several patients three or four times. They do not even intimate, that ever a second, third, or fourth inoculation has infected a patient of theirs with the small pox: much less, that any one ever took the distemper, after inoculation, in the natural way. Indeed, it is notorious, that
hardly

hardly ever either of these accidents have happened in the new way of inoculation. I myself will, on the strength of my own experience only, undertake to say such accidents are extremely uncommon. And I am well satisfied, if Doctor Langton had known of but *one* such, he would have informed the publick of it before now. I do not say, that it is not possible for such a thing to happen. This, however, I can safely say, that it can scarce happen, but by *the ignorance* or negligence of the inoculator. The conclusion I would draw from the premises is, that the above gentlemen acted very judiciously, in inoculating their patients several times, when they had the distemper so extremely lightly, not only to satisfy them, that they had passed through it, but also to take away all possibility of a natural infection happening afterwards; and likewise that Doctor Langton has acted very unfairly in inferring from what they have said on the occasion in their advertisement, that *second infections are no unusual things*.

I hope the Doctor is by this time fully convinced, that he has drawn a very unfair conclusion from some of the passages occurring in the advertisement he has cited. I shall now therefore proceed, and take notice of another objection he endeavours to urge against the practice. This
indeed

indeed is a very trifling one, as most of the others are.

He observes in the third page of his pamphlet as follows, *the greatest physician that ever lived, could never foresee that the real benign small pox would assume the appearance it now exhibits on the inoculated, and for the truth of this I appeal to the silence of medical writers on this head.* From this observation, the Doctor I suppose, would mean to insinuate, that the distemper communicated in the new way of inoculation, is not the genuine small pox, or I cannot see, what business it could have, where it is inserted. This is a far fetched deduction indeed. Alas! what has this to do with shewing the present practice of inoculation to be useless? As well might the Doctor endeavour to demonstrate the inutility of the many discoveries and improvements, that have been made in the several arts and sciences within these few centuries, because truly those, who formerly wrote of such arts and sciences, did not foresee these discoveries and improvements would be one time or other made. Who, for instance, of the ancients ever dreamed of the improvements and discoveries, that have been made within this century, in astronomy? Who of that surprizing discovery of the composition of gunpowder? Yet the Doctor would not from thence, I hope, conclude against the reality or utility of such discoveries and improvements.

provements. But this is not all. The observation itself is not, strictly speaking, true. The great Doctor Boerhaave, a physician, whose authority the Doctor himself has cited, did certainly, in some measure, foresee this event. We have seen above, that this great man held an opinion, that the *natural small pox often happens without pustules*. I shall now, in order to shew the Doctor, that medical writers have not been altogether *silent* on this head, translate and cite a few passages of that author. Doctor Boerhaave treating of the small pox says, in the beginning of the eruptive fever and symptoms, **this seems to be the first indication of cure, that the inflammatory stimulus being removed, the distemper be cured in its present state, its farther progress obviated, and so suppuration, gangrene, and the like prevented. It seems possible to remove this stimulus by correcting it by specifics so called, or else by the common antiphlogistick method. This specific correction of the variolous stimulus seems to depend on finding a remedy to counteract the variolous venom, which though received into the body in a very small quantity, yet produces the inflammatory symptoms as its effects. That it is possible for such a specific remedy to be discovered, we have reason to hope from getting a history of antidotes, and from the nature of the distemper itself; and the extreme great utility likely to accrue to mankind from such a discovery is enough to incite men to*

* Vid. Boerhaav. Aphorism. § 1388, 1389, 1390, 1391, 1392.

attempt

attempt it. The success that has sometimes attended the use of antimony and mercury is sufficient to encourage us to seek for such a remedy, in these medicaments brought by art to an high degree of penetrability, and deprived in good measure of all saline acrimony, but well united. So far the great Doctor Boerhaave. From whence it is very evident, that he not only foresaw, that the natural small pox might very probably be rendered extremely light in the eruption, by medicines administered even after the eruptive symptoms were begun, but also that he pointed out the very medicines, which are now used as alteratives with such surprizing success in the new method of inoculation. Neither was Doctor Boerhaave the only medical writer, who was of this opinion. Doctor Hillary, a very eminent physician, and a good writer on the subject, was of the same way of thinking, as may be seen in the essay annexed to his practical treatise of the small pox. And the late Doctor Lobb, if I remember right, has asserted, that he has actually prevented the eruption of the small pox by a mercurial medicine administered during the eruptive symptoms of this distemper. Thus much may serve to show of how little use the last cited observation of Dr. Langton's is to him in his endeavours to demonstrate the distemper communicated by the present method of inoculation to be other than the small pox, and the practice of this method to be useless.

I must

I must beg leave next to take notice of a passage in the Doctor's pamphlet, in which he has treated Doctor Baker, a gentleman who has written on the subject, with an hundred times more candour, and temper, and I had well nigh added also, infinitely more like a scholar, than himself, in rather too cavalier a manner. The passage I mean is this, **if Doctor Baker imagines the cool regimen was discovered by Sydenham, he errs for want of information.* This passage naturally reminds me of an old English proverb concerning teaching one's grandmother to suck. If Doctor Langton thinks, that Doctor Baker *imagines Sydenham to be the discoverer of the cool regimen* in the small-pox, I, who never exchanged a word with Doctor Baker, nor ever saw him, that I know of, will venture to affirm, that Doctor Langton *errs for want of information* himself. Doctor Baker is a physician of sound learning, and extensive knowledge in his profession, as may be seen even in that tract of his, to which Doctor Langton alludes. And if he doubts in the least the truth of what I say, I refer him to another treatise of Dr. Baker's, namely, that *de affectibus animi & morbis inde oriundis*, in which he will see such proofs displayed of this gentleman's classical erudition, and extensive reading, as will not fail to convince him, that he could hardly be unacquainted with the Latin version of *Rhazes on*

* Vid. Dr. Langton's Address, &c. P. 6.

the small-pox and measles published a few years since by the learned Doctor Mead.

The next passage in the Doctor's pamphlet, which I shall take notice of, is that, wherein he asserts, that *† did physicians more frequently use plenty of air and of cooling medicines, such a lamentable carnage would not be made by the disease in the natural way.*

The Doctor's view, I suppose, in introducing this observation, was to persuade the publick, that *cooling medicines*, and *the free use of air* in the natural small-pox, are of themselves sufficient to save almost every patient, and that consequently inoculation is in a great measure needless. If this be the case, as I verily believe it is, I must beg leave to dissent from him in opinion. That plenty of cooling medicines, and the free use of cool air, are very good remedies in the natural small-pox, I most readily allow. But then it is a fact very notorious to practitioners conversant in the cure of this distemper, that there are kinds of it so very malignant, that oftentimes neither the use of these, nor of any other remedies, hitherto published, can save the patient. In and very near the Town I live in, within these eight or nine months past, perhaps, about twenty adults have had the

† Vid. Dr. Langton's Address, &c. P. 8.

small-pox in the natural way, all of whom had, I understand, plenty of cool air, and of cooling medicines administered to them. Yet of this number six died. Of these twenty patients, five, who were healthy and young enough, took the distemper from one, who died of a most malignant kind of it contracted in the natural way : and three of the five died, notwithstanding they had, so far as I can learn, all the assistance, which cool air, and cooling medicines could *possibly* afford them. *Such a lamentable carnage*, to use Doctor Langton's own expression, may serve to shew, that inoculation is not likely to be much less necessary, now that the cooling regimen is so universally adopted, than it was before. One thing I believe I may venture very safely to say it tends to *shew*, an opinion, in which most sensible practitioners will readily join me, and this is, that, had these unfortunate people been inoculated in the new way by a *judicious practitioner*, in all probability they had been alive now.

Here too I must beg leave to dissent from the Doctor with respect to another particular assertion of his in the pamphlet before me. He says, * *when either the benign, confluent, or putrid small-pox seizes a family, it preserves its type, how different soever the subjects may be in age and constitution.* I am much mistaken, if this doctrine be agreeable to

* Vid. Dr. Langton's Address, &c. P. 13.

experience. It is true, the natural distemper is apt to handle some families much more severely than others; a circumstance, of the cause of which we are totally ignorant, unless it be owing to this, that the blood of some families contains, hereditarily, a greater quantity of such principles in it, as are easily assimilated by the contagion of the small-pox. Be this as it will, it is very common for the distemper, when it breaks out in a family, to affect the several persons, of which it is composed, with very different degrees of severity. One perhaps sickens of the bleeding pock with petechiæ, another of the confluent kind without petechiæ, a third of a kindly distinct sort, and so on. The truth of the matter is, the nature of contagion of the small pox is such, that it matters not, I believe, in the least, from what kind it is taken, in respect to the production of this or that kind in the person infected. The most malignant small pox often excites in the patient infected from it a fine distinct kind, and so on the other hand, a fine distinct pock often excites the most malignant. So true is the observation, which the learned Doctor Mead made many years ago, when treating of inoculation, * *it is of much greater consequence in my opinion, into what kind of a subject the virus of the small-pox is infused, than from what kind it is taken.* Indeed, were not this the case, inoculation of every

* Vid. Mead de Variolis et Morbillis, P. 84.

kind

kind were in a manner useless. For it is altogether by the alteration induced on the body of the patient by the medicines he takes, and the regimen he observes, during the process, that inoculation is enabled to render the distemper so light. And I will be bold to say, that the present method of inoculation is in this respect so very efficacious, that if patients are treated *judiciously*, and are themselves observant of the directions given them, the distemper will prove very light, nine hundred and ninety-nine times out of a thousand, let them be inoculated from what kind of pock they will, unless, perhaps, we except the bleeding kind, and I do not know that even this is to be excepted. I have, at this very time, an * adult patient under my care, upwards of forty years of age, who was inoculated from one of the two surviving patients of the five above-mentioned, who has the small-pox in a most favourable manner, having no more than about twenty pustules. So that Doctor Langton might very well have spared himself the trouble of writing all he has written in the thirteenth and fourteenth pages of his pamphlet concerning *contagion*. The contagion of the small pox is ever one, and the same, and only produces different effects, according to the difference of age, habit, constitution, and other circumstances, in the person infected by it; and if the Doctor meant to insinuate by

* This patient was inoculated with yellow well concocted matter:

the observation he has made on *contagions*, that the distemper communicated in the present method of inoculation is too light to be the small pox, he is as much mistaken as ever he was in his life, as will be shewn at large below.

I come now to consider the Doctor's grand argument in behalf of the doctrine advanced in his title page. He says, **I think it may be laid down as certain, that a disease is to be esteemed the same universally, when it presents the same appearances, and vice versa; and that any deflection from these appearances must bring it under a different denomination, and have different indications of cure.* So far the Doctor. We will now see, how far this observation is applicable to the distemper, to which he applies it. I am here ready to ask, what are these same *appearances* in the small pox, *a deflection from which must bring it under a different denomination?* One variolous patient has a large distinct kind of pock, another a small confluent kind, another a verrucose kind, another a bleeding kind, another a crystalline kind, another has no pustules at all, and so on. And yet the distemper is the small pox in all of them. Here is *deflection* enough with a witness. To say the truth, this doctrine of the Doctor's is by no means applicable to the small pox; nor indeed to any other disorder, except it

* Vid. Dr. Langton's Address, &c. P. 8.

be such a one, as has one or more pathognomonic symptoms belonging to it, of which kind there are very few, and which is far from being the case of the small pox. If it were not, this distemper would not so often be mistaken, as it is, by injudicious practitioners for other disorders. But let us see, how the Doctor applies the above doctrine himself. **Let us, says he, by this clear and plain rule, examine the natural and inoculated small pox. Patients seized with the first kind, have a chillness succeeded by a feverish heat; a pain in the head, back, and stomach, a nausea, and restlessness—The pustules are prominent, and filled with well concocted pus, inclining to yellow; there is a considerable fever, and the patient often light headed. These symptoms ever attend, in a greater or less degree, the benign small pox, and infallibly preclude all future infection.* He goes on, *On the other hand the inoculated small pox (if it deserves that name) has not one of the preceding symptoms, because the inoculated matter is not the same with the true small pox.* How any man in so respectable a station as Doctor Langton's profession manifestly places him in, can have the assurance to publish an assertion so diametrically contrary to truth, and the experience of thousands in this kingdom, is most astonishing. Surely, he ought to have made some little enquiry into the state of the matter, before he had ven-

* Vid. Dr. Langton's Address, &c. Page. 8 and 9.

tured to have written about it in this positive manner. But, alas! Dr. Langton is not the first man, whom prejudice has hurried far out of the limits of right reason. Be this as it will, I will undertake, on the strength of my own experience in the matter, to declare, that patients inoculated in the new way, instead of *not having one of the symptoms*, which he has enumerated, *have*, for the most part, by far the greatest part of them, as I have observed in hundreds. These patients are generally *seized with a chillness, pain in the head, back, and stomach, are restless, have a fever, and are sometimes light headed*; and these symptoms are almost always followed by an eruption of pustules which grow *prominent*, and gradually *fill with well concocted pus*, in colour *inclining to yellow*. I affirm this to be true, and the Doctor himself confesseth, that *these symptoms infallibly preclude all future infection*. So that after all, the Doctor's grand argument against the present method of inoculation has met with the dire mischance of being refuted even by his own acknowledgement,

The Doctor soon after observes, by way of discrediting the new method of inoculation, that
 * *the present practice is to take the matter from the incision, the fourth day after the incision is made.*

* Vid. Dr. Langton's Address, &c. Page 10.

By

By this means, says he, you have a contagious caustic water, instead of laudable pus, and a slight ferment in the lymph is raised, producing a few watry blitches, in the place of a perfect extrusion of the variolous matter. It was, without doubt, the practice of Mr. Sutton to inoculate from the punctures on the arms of his patients, while the matter in them was yet crude, and before the eruptive symptoms came on. And it seems, as if he looked on this as a necessary caution, in order to render the distemper so inoculated, light, on the patient. Be this as it will, farther experience has abundantly demonstrated, that this caution is altogether useless. It is not of the least consequence in the present method of inoculation, whether the patient be infected by the application of crude variolous lymph, or yellow concocted variolous matter. Nor does it signify in the least, whether this variolous matter be of the inoculated or natural kind. The same appearances constantly follow, and the patient, if he is *judiciously* treated, and is regular himself, ever contracts a kindly distinct kind of the distemper. Here too the Doctor's prejudice has led him wide of his mark.

The Doctor soon after observes, * *in the inoculated, the pock being nothing but clear lymph, is soon re-absorpt by the cutaneous glands, and disappears in a few*

* Vid. Dr. Langton's Address, &c. P. 10.

hours.

boars. And he adds, *from this very symptom it can be demonstrated, that the matter at present inoculated is by no means variolous.* I have shewn above, that the pocks mature in the inoculated in the new way, as they do in patients under the natural small pox. Therefore, as to what he here says of their being *reabsorbed by the cutaneous glands, and disappearing in a few hours,* it is altogether without foundation, and contrary to the most notorious facts, And I will defy him to shew the distemper so inoculated to be different from the natural small pox, either in the maturation, exsiccation, or even the desquamation of the pocks, or indeed in any other circumstance, the lightness only excepted. But to say the truth, he seems to know little of the matter.

The next passage I shall take notice of in the Doctor's pamphlet is that, wherein he would insinuate, that because * *the exposing the inoculated to very cold air in a rigorous season, is not found injurious,* therefore the distemper they labour under is not the genuine small pox : for he adds, *such a practice would be mortal in the real small pox.* I would here ask the Doctor, whether he thinks, *the hundred and twelve patients,* which, Professor Monro says, *were inoculated in the middle of the winter, in some of the most northern isles of Britain, where there was scarce*

* Vid. Dr. Langton's Address, &c. P. 12. † Vid. Baker's Inquiry, &c. p. 49—50.

fewel enough to prepare victuals, many of whom went abroad barefooted in snow and ice, and who notwithstanding all lived, had the genuine small pox. For my own part I do not doubt it at all, having seen many patients under inoculation in the new way going abroad with variolous pustules on them in such weather, without receiving the least injury from it. And I can assure the Doctor, that the cold weather is not in any wise so injurious as he seems to imagine, to patients under the natural small pox, even where they have a very considerable eruption. I have known several such patients go out very frequently in the space of a day into the air, when the weather has been both cold and wet, without receiving the least injury from so doing. So that this argument of his is of as little signification in proving the distemper to be other than variolous, as the rest.

The Doctor soon after observes, as follows, * *I do believe that at first the real pock was communicated, but after prevented from taking proper effect by the interposition of doses of mercury and antimony. Then I suppose Mr. Sutton at first inoculated his patients with the true small pox ; but it has at length, it seems, by the interposition of his alterative medicines, degenerated into the variolæ nothæ, a true bastard kind of small pox. I never heard any thing of*

* Vid. Dr. Langton's Address, p. 16.

this

this same bastard kind, but it at length, to the sorrow of some one or more, proved the true kind. The truth of the matter is, the small pox Mr. Sutton, and other inoculators in this new way, communicate at this very day, is as much the small pox, as ever it was, as several have lately experienced to their sorrow, who have been unlucky enough to be infected by their patients. Mr. Bromfield, I think, has related an instance * *of the inoculation of twenty children at one and the same time from a person, who was the fourteenth in descent from the natural small pox, which children so inoculated all took the distemper, and some of them indeed pretty severely,* and a very ingenious surgeon of my acquaintance, Mr. Reid of Chelsea Hospital, who, I firmly believe is as well qualified, as any man in the kingdom, to carry patients safely through inoculation in this new way; assures me that he has translated the variolous matter, by inoculation from one person to another, even to the thirtieth repetition, and yet the last has had the small-pox, just as manifestly as the first had. I would fain ask Doctor Langton, how it happened in these cases, that the small pox, after having passed through so many successive translations from one to another, did not at length degenerate into a bastard kind. I suppose he would say, because the several patients did not take *mercury and antimony*. Some

* Vid. Bromfield's Thoughts, &c. p. 10, 11.

of them very probably did. But to say the truth, it matters not to the point in question, whether they did, or not. Let patients take what they will, the contagion of the small pox communicated to their blood will ever excite the small pox in them, and it will continue to do the same, even if it be translated from one to another ad infinitum. And I cannot say, but I am much inclined to think, there is not a physician in the kingdom, besides Doctor Langton, who does not firmly believe, that the distemper communicated in the present method of inoculation is no other than the true genuine small pox. Nor can I forbear thinking *his* behaviour on this occasion equally as perverse, as was that of those physicians of old, who, as Galen informs us, * *because they were unable to assign the causes of things, which were self-evident, denied the very existence of such things altogether.*

I have now, I think, gone through with every argument and objection of the Doctor's against the new method of inoculation, that is worthy of the least notice. As to the affair of the inoculation in *France*, I do not pretend to know any thing of it. It is foreign to my purpose to inquire into the merits of *that*, as I am defending the cause of the

* Vid. Gal. de loc. affect. l. 5. c. 2.

inoculation practised in *England* only. And I hope I have fully convinced the Doctor by this time, that however the case may have been in *that* country, the distemper, which has been of late inoculated with surprizing success in *this*, is the true small-pox. Lest I may not have succeeded, I must beg leave to mention a fact or two, which, I will venture to say, carry, each of them, infinitely more weight with them in proof of it, than all the arguments and objections he has urged, carry in disproof of it, taken together.

The first I shall mention is this. Out of a great many hundred persons of my acquaintance, who have been inoculated lately in this new way, many of whom have visited patients ill in the most infectious stages of the natural small-pox, and several of whom have assisted in carrying patients, who have died of the distemper in the natural way, to the ground, I do not know one who has taken it: a thing, which must inevitably have happened before now, amongst such a number, had the disorder communicated to them by inoculation been other, than the true small-pox. I will not presume to say, but such an accident may happen one time or another. It has happened in the old way of inoculation. And it is still more likely to happen to patients in the new, if they are inoculated by *ignorant* practitioners. All I contend for at present is, and this I affirm to be true,

true, that the distemper communicated in the new way of inoculation is the *genuine small pox*, and will secure the person once infected with it ever after from taking this distemper. The other fact I meant to mention on the occasion is the following. It is a circumstance well known to a multitude of persons in this kingdom, that patients inoculated in the new way have several times accidentally communicated the contagion of the true small-pox to others, many of whom have had the distemper very severely. The deduction from this is obvious. Such inoculated patients *must* have been under the true small-pox themselves, or others *could* not *possibly* have caught it from them.

Far be it from me to offer to make any remarks on Doctor Langton's diction and inaccurate manner of writing, as this might be esteemed a task rather invidious, and foreign to my professed purpose of *vindicating the new method of inoculation*. I cannot, however, forbear observing on the occasion, that the Doctor is, I must needs say, highly blameable for having written with so much prejudice and rancour against a practice of the merits of which he is manifestly no competent judge, and which, for aught he knows, or is able to prove to the contrary, is well calculated to be as useful to mankind, as any discovery ever yet made by the ingenuity of man. Nor can I take leave of his performance without declaring it to be my sincere
 opinion

opinion, that notwithstanding all the arguments and objections the author has urged against it, inoculation in the new way still merits the attention and regard of the public equally as much as it did, before he published on the subject at all.

I come now to consider the arguments and objections urged against the new method of inoculation by Mr. Bromfield. And here, before I go any further, I cannot but in justice to this gentleman observe, that I believe he has published his thoughts on this subject with no other intentions than *those of doing good to mankind*. Doctor Langton has evidently written on the subject in a positive magisterial strain. On the contrary, Mr. Bromfield offers his thoughts on it in general in rather a modest diffident manner, and has expressed his disapprobation of the practice, rather under the form of doubts and fears, that it is not a good one, than in an haughty dogmatical strain. It is natural I believe, for every man to be more or less prejudiced in favour of his own method of proceeding in most cases, when set in competition with that of another. Mr. Bromfield will pardon me I hope, when I observe, that he is, I fear, rather too much prejudiced in behalf of his own method in the practice in question. Perhaps, I myself, am not without prejudice in favour of the new method of inoculation. Of these matters, we must leave indifferent judges to determine.

termine. Be this as it will, as Mr. Bromfield has declared himself * *open to conviction*, and seems very desirous of finding out the † *most safe method of inoculating*, if I can be of any service to him in his inquiry, I shall very readily contribute all, that is in my power to assist him on the occasion.

The first ‡ objection of Mr. Bromfield's, which I shall take notice of, is that which he makes to the method of *operating* in the new way, I have, I hope, already abundantly shewn, that this method is much superior to the old one in point of certainty of infecting the patients; a thing by the bye, which is of no small importance in inoculation. I shall now endeavour to shew, that it is preferable to the old one in some other respects besides. In the first place, supposing the puncture to be made as it ought, with the lancet held in a very oblique position, there is not so great danger of too great an inflammation supervening on the adjacent parts, in this way, as in the other. I declare in six or seven hundred cases of patients inoculated in the new way, I have not seen one, in which the inflammation on the arm was considerable enough, to excite such a degree of pain, as was scarce worthy of notice. Indeed in most cases, the inflammation and tumefaction of the skin round the punctures

* Vid. Bromfield's thoughts, &c. P. 4. † Vid. ibid. P. 44.

‡ Vid. Bromfield's thoughts, &c. P. 11.

extend

extend only to the distance of about an inch at the time of the eruptions appearing, and in a day or two after they generally begin to abate. Now and then it must be confessed they extend somewhat farther. But I have always found, whenever they became in the least troublesome, the application of sweet oil, ceratum album or some such cooling medicament, has never failed of giving relief. In the next place, I am strongly of opinion, that one may determine sooner and better, whether the operation has taken place, when it is done by puncture, than when by incision; a circumstance, that is not without its uses. Lastly, I am much inclined to think, it is the better and safer way, to apply nothing at all, as is the custom followed in the new way, to the part, into which the variolous matter is infused, and to let the infection take its natural course on it, unless in cases, where the circumjacent skin is too much inflamed. Pustules are apt enough to break out pretty thick, at the time of the patients sickening, about this part even in the new way of inoculating; and when they do, they hardly ever fail, if numerous, of inflaming the adjacent skin, and of exciting rather more pain, than the patient could wish for. Mr. Bromfield will, I hope, excuse me in dissenting from him in opinion, with respect to *the application of hot pultices* to the part infected.

• Vid. Bromfield's thoughts, &c. P. 14.

I own, I cannot see any use they can possibly be of. They may indeed by their heat excite a much greater number of pustules, and consequently a much greater degree of inflammation round about the incision, than there would otherwise be; circumstances, that will be troublesome, not to say dangerous to the patient. But the question is, whether the patient will have the fewer pustules in consequence of these their effects on other parts of his body. For my own part, I believe not a *single pustule* the fewer; in a word, I am of opinion, that the application of *hot pultices* to the parts infected in inoculation of the small-pox is, in some measure, akin to the use of other heating remedies in the distemper, and ought to be discarded along with them, as a suspicious remedy at least. To say the truth, I am strongly of opinion at present, that the new method of inoculation cannot in this respect be possibly mended. If I am wrong, I am sorry for it. One thing I am certain of, and this is, that the part infected inflames much less, and heals much sooner in general, when the operation is performed by puncture, than when it is done by incision.

Mr. Bromfeild seems to make no doubt, but there is * *an advantage arising from the discharge of the incisions*. Far be it from me to deny, that the

* Vid. Bromfeild's thoughts, &c. P. 15.

discharge from these ever draws off any of the variolous venom out of the blood of the patient. This, however, I cannot forbear thinking, that the *advantage*, that can arise from this to the patient, is very trifling indeed, and in no wise compensates for the other inconveniencies, and even dangers that may arise from performing the operation by incision, and keeping *hot pultices constantly applied round* that, and the neighbouring parts. Sure I am, I have several times seen patients inoculated in the old way by incisions, which have discharged a great deal, and yet the patients have been covered with the small-pox from head to foot, and have for many days been in the utmost jeopardy. The truth of the matter is, it is not of any consequence at all to the patients having the distemper lightly or severely, whether he be inoculated by incision or by puncture. These events depend altogether on quite different circumstances. However, on the whole, I cannot but give the preference to the new method of operating, and as to the *continual application of hot pultices*, I think the remedy can do no good, and may possibly do much harm.

The next objection of Mr. Bromfeild's, which I shall take notice of, is that, wherein he says, * *to risque the patients receiving the infection by the*

* Vid. Bromfield's thoughts, &c. P. 13.

mouth and nostrils, in my opinion, must be wrong, or otherwise inoculation is absurd. Here I can neither agree to the premises, nor to the deduction. That there is no manner of danger accruing to the patient, from his being in the same room with, and close by, the variolous patient, from whom he is inoculated, the great and surprizing success of Mr. Sutton's mode of operating evinces to a demonstration. And as Mr. Bromfeild himself allows ** facts are very stubborn things*; it is, therefore, needless for me to make use of any argumentation here. But supposing it makes no difference to the patient, as to his having the small-pox lightly, or severely, whether he be inoculated, or takes the distemper by *the mouth and nostrils*, as I verily believe is the case; does it therefore follow, that inoculation is absurd? Most certainly it does not. Where a patient has gone through his preparation, and is determined to run the risque of inoculation, in order to secure himself ever after from the natural distemper, he would most certainly chuse to receive the infection in that way, which is the most sure to communicate the small-pox, and which also best shews the progress of the infection. And it is certain, that inoculation, especially in the new way, is in these respects far less liable to deceive his expectations, than going into an infected room. Inoculation, therefore,

* Vid. Bromfeild's Thoughts, &c. p. 51.

is not absurd, but is a very rational practice, even if this supposition be true.

Mr. Bromfeild says, * *an inflamed state of the blood is certainly a great objection to the operation being done, as experince has long since determined.* I scarcely know what Mr. Bromfeild means by *an inflamed state of the blood*. It seems to me a vague indeterminate kind of an expression. If he means a fizy state of it, I must beg leave to dissent from him. I remember some years ago a very judicious and experienced practitioner bled a patient under preparation for inoculation, and finding his blood very fizy, observed on the occasion, that he would probably have the distemper very severely. The event however shewed he was mistaken in his prognostick, The patient had the distemper very lightly, indeed much more favourably, than any one of a large company who were inoculated with him. To say the truth, in the present way of inoculation, the state of the blood is of little consequence, so very powerful is the method pursued in it, in subduing the disorder. If the viscera are but sound, and the health of the patient sufficiently good, to enable him to go through the discipline required, without its being impaired by it, it is sufficient. It is scarce possible for him under

* Vid. Bromfeild's Thoughts, &c. P. 6.

this method, unless it be his own fault, to suffer much from the distemper itself.

I shall next take notice of Mr. Bromfeild's objections to the use of * *mercury in the preparation for inoculation*. And here I cannot say, but I must so far agree with him, as to think mischief may possibly arise to the patient from too free, or injudicious an use of this medicine. How far mercury counteracts the variolous principles in the blood by any specifick virtue, must be left to farther experience to determine. For my own part, I must confess, I have not been concerned in the inoculation of a single patient, who has not taken more or less of this medicine during the process. And yet they have all done extremely well. I am, nevertheless, sufficiently aware, that it ought ever to be used with *caution*, having been consulted for some patients ill after inoculation, whose disorders, I have the utmost reason to suppose, arose altogether from the *injudicious* administration of this medicament. Nor are Mr. Bromfeild's fears of a dissolution of the blood ensuing from too liberal an use of it without some grounds. So far as I have hitherto been able to determine, this medicine may be used very freely, and to advantage, in strong robust constitutions, and gross corpulent habits. In more delicate and thin patients greater

* Vid. Bromfeild's Thoughts, &c. P. 7.

caution and judgment must be used in administering it. Otherwise, mischief may be done. There are, moreover, as Doctor Baker very judiciously observes, * *certain constitutional antipathies with respect to this medicine*. Whenever, therefore, a practitioner observes, that it disagrees, either by throwing out a miliary eruption, by affecting the fauces, by exciting a feverishness, or the like, it is by far the most prudent way for him to discontinue the use of it, or at least to intermit its use for a few days, and to trust to the other remedies, which we shall see by and bye are of themselves very powerful. I am not ignorant, that many inoculators look upon mercury as possessed of a certain antivariolous virtue. And they support this opinion by a notion, that those patients, who have been accidentally salivated under inoculation, have ever had the small pox in the most favourable manner. I will not take upon me to deny, that this may have generally been the case, since I much approve of the use of mercury in inoculation myself. All I contend for is, that it ought ever to be administered with *prudence and caution*: for I am certain, I have more than once, twice, or thrice seen very ill effects arise from its having been given too liberally to patients under this process. At the same time, it must be confessed, that it is no proof of the general inutility of a me-

* Vid. Baker's Inquiry, &c. P. 25.

dicine in any disorder, because such medicine *injudiciously* used has had ill effects on some certain people under such disorders, for, as the poet well observes,

* *Nil prodest, quod non lædere possit idem.*

And here it may not, perhaps, be an improper place, as it may serve to throw some light on the use of mercury in inoculation, to inquire into the causes, which render the small-pox so extremely light in Mr. Sutton's method.

It is the opinion of Mr. Chandler of Canterbury, that this event is owing † *principally* to the patients being inoculated with *crude variolous lymph*. I am in a manner certain, that this circumstance is entirely out of the question here, having seen a *great number* of persons inoculated in the new way with well concocted yellow matter, taken from the natural, as well as the inoculated small-pox, all of whom have had the distemper full as lightly as those, who have been inoculated with crude variolous lymph. If I might be indulged with the liberty of making a gross comparison, I would here compare the nature of the variolous feeds, or principles, in the blood to that of gun-

* Vid. Ovid. de trist. Lib. 2. V. 266. † Vid. Chandler's Essays, &c. P. 41.

powder, and the action of the variolous venom infused into the blood on those seeds, or principles, to the action of fire on that composition. It is notorious, if fire be applied to this composition, that it immediately deflagrates it, whether this fire be weak or strong, whether it be in a small quantity or a large one. It makes no difference as to the deflagration itself, whether the fire applied be a single spark from wood, or whether it be a red-hot piece of iron. Just so I take it to be in inoculation, with respect to the crude variolous lymph, or concocted variolous matter, on the principles of the small-pox in the blood. The effects the matter produces in deflagrating, if I may so say, the variolous principles, are just the same, whether it be crude or concocted whether it be weak or strong. If it be but strong enough to deflagrate them at all, it is sufficient. For it is the operation of the regimen and medicines used in the new way of inoculation on the patients body, that renders the small-pox so extremely favourable, and not the quality of the infecting matter as to crudity or ripeness. Water mixed with gun-powder, to continue my comparison, has not a more sensible effect, in preventing a violent deflagration of this composition on the application of fire to it, than the SUTTONIAN regimen and medicines have, in preventing a violent deflagration of the variolous principles, in the blood of an inoculated patient. But of this question enough has been said already.

It

It is the opinion of Doctor Glas, that the circumstance of Mr. Sutton's patients having the small-pox so extremely lightly, is owing *principally* to the administration of sudorifick * *medicines during the eruptive fever*. I have a very high opinion of the abilities of Doctor Glas, in matters of medicine in general, but I must beg leave to dissent from him here. The medicines used by Mr. Sutton during the eruptive symptoms in inoculation seem to be no † other, than Clutton's febrifuge spirit or tincture added to water in a certain proportion. And it must be confessed, that this medicine, especially if it be given warm, seems well calculated to excite sweat. This composition I have used with the utmost success, during the sickening of patients under inoculation. But then, on the other hand, I have many times observed, that those patients, who have not taken a single drop of this medicine, but have drank only weak tea, or lemonade, or some other cooling diluent liquor, have been equally as inclinable to sweat, as the others, and have had just as favourable an eruption too, as they have. To say the truth, the business is done, before ever the sweat comes on, by the operation of the regimen and medicines used in this method of inoculating: and the patient, if judiciously treated, will necessarily have the small-pox light, whether

* Dr. Glas's Letter to Dr. Baker. † N. B. I except Purgatives.

he takes Mr. Sutton's *punch*, as it is vulgarly called, or drinks only cooling diluent liquors. As to the disposition to sweat, it comes on naturally, and of itself in this way, as well as in the old way of inoculation in most cases of adults on the decline of the eruptive symptoms. And as there is the utmost reason to suppose it to be critical, at least with respect to these symptoms, no doubt is to be made, but it ought to be encouraged with moderation, by the use of cooling diluent liquors, administered more or less warm, according as the weather is more or less cold.

It is the opinion of the learned Doctor Baker, that the * *principal advantage of the new method of inoculation, is derived from the free use of cold air during the whole process.* It must be confessed, that this particular part of the new method is not without its uses: nor can it be denied, that they are very considerable. I cannot, however, think with the Doctor, that the *principal advantage is derived* from thence. And as I doubt not, but he is open to conviction, I will give him my reasons for my opinion. I have seen several patients, who have scarce ever stirred out of doors during the whole process in this way of inoculation, when the weather has been cold, and who have, nevertheless,

* Vid. Baker's Inquiry, &c. p. 28.

had the small-pox extremely favourable. Others I have seen, who have kept their beds almost altogether during their sickening, in such weather, and yet have not had so many as twenty pustules. But then I must confess, these patients have not been heated, either by a load of bed-cloaths, or by having their curtains drawn close. And I never saw scarce a single patient, let him have taken all the benefit he possibly could from the use of cold air, who eat any considerable quantity of animal food, at any time during the process, but he had a much larger share of variolous pustules, than those, who refrained from such food, and were managed in all other respects, as such patient had been. To say the truth, animal food of every kind seems to convey into the blood a deal of fuel; or pabulum, which is easily assimilated by the contagious humour of the small-pox. And it is hardly to be doubted, but one grand advantage derived from the new method of inoculating, arises from the patient's being denied the use of such aliment. In the old way, it was the custom of many practitioners to allow their patients to eat, as often almost as they pleased, of butter, cream, mild cheese, weak broth, and the like, with now and then a dinner of white flesh-meat. And some have indulged them still farther, and allowed them to eat such dinners every day, and to drink a glass or two of wine besides. In the new way, the custom is to debar the patient, during the whole

whole process, from eating flesh, butter, cream, cheese, eggs, or any kind of animal food, skimmed milk only excepted. Here then, we see, is a most striking difference between the two methods: no less striking, than the general severity of the distemper in the old way, and its general, or rather universal, lenity in the new. Now, that one grand cause, why patients inoculated in the old way were apt to have the small-pox much more severely, than those inoculated in the new, was owing to their being indulged with the liberty of eating very considerable quantities of animal food, is farther evident from hence. If a patient be never so rigidly prepared, and managed, during the whole process in the new way, so that he passes through the sickening without the appearance of a single pustule, yet if he eats any considerable quantity of animal food at this time of the disorder, it will scarce ever fail to throw out a considerable number of pustules. This experiment I have known tried several times, where patients have been very desirous of having a few pustules: and I do not remember, that it ever failed of producing the desired effect: a strong proof this, that animal food conveys into the blood, as it were immediately, a deal of pabulum, which is easily assimilated by the variolous venom. I might here insist on patients being denied the use of spirituous and fermented liquors, together with that of spices, in the new way of inoculation, as an additional

ditional cause, why such patients have the small-pox so extremely lightly. But, though I make no doubt, that a total abstinence from these, and from every thing else, which is apt to heat the blood, contributes very considerably to render the distemper light, yet I cannot forbear thinking, that one grand cause of this event is the patients total abstinence from every kind of animal food. And, if I am right in my opinion, we may here see the principal cause, why the natural small-pox is very often apt to prove pretty severe even on persons, who are abstemious enough in all other respects, save that of eating freely of such food. But this circumstance of the abstinence from animal food enjoined patients in the new way of inoculating I do not look upon to be the only grand cause, that renders their distemper so very light. Another cause of equal efficacy towards producing this event I take to be the very frequent and brisk purging, which they undergo during the whole process.

In the old way of inoculation, it was the custom to purge the patient much more mildly, before the operation, than he is wont to be purged in the new. And in that way, after the operation was once performed, the practitioner refrained, as it were religiously, from all purgatives, lest he might thereby raise a commotion in the blood to the detriment of the patient. Here I think the new
method

method has an inexpressible advantage over the old. Practitioners in the present way purge their patients not only more briskly before inoculation, than the practitioners in the old used to do, but they likewise keep on purging them at due intervals afterwards, even till after the eruptive symptoms supervene. Now, if there be any one disorder in the whole catalogue of ills incident to us frail mortals, which is in its own proper nature inflammatory, certainly the small pox is such. Accordingly we find by experience, that all those causes, which are apt to aggravate other disorders of an inflammatory nature, are apt also to aggravate this, whether they are brought into action, before the patient sickens, while the variolous venom is silently and secretly operating on the mass of humours. or after the eruptive symptoms are begun. The distemper itself, then, being inflammatory, and the remedies here spoken of being two of the most active and powerful, which the art of medicine affords, in subduing disorders of an inflammatory nature, it cannot, surely, be a matter of any considerable surprise to a rational and judicious practitioner, that the small pox in the present way of inoculating is very frequently extremely light. And he will certainly see still less reason for surprise, if he considers, that these remedies are ever assisted in their operation by others, which, though of an inferior degree of efficacy, are not without

without their use; and that all these remedies together are almost ever brought into action on the patient for several days, before the variolous venom is infused into the blood, and continue in action, even till after the sickening begins, and the greatest part of them, till after the eruption is finished. We see, the small-pox sometimes proves pretty favourable even in the natural way. It must of necessity then, one may reasonably think, considering the extreme efficacy of the remedies applied in the new way of inoculating, very often prove extremely light on patients inoculated in this way. That brisk and frequent purging, as well as a rigid abstinence, is of extremely great efficacy in subduing the violence of the distemper in the new method is, I think, very manifest, not only from hence, that great numbers of persons of all ages, habits, and constitutions, have been inoculated with the utmost success, even without any preparation previous to the operation, but also from the good effects of it in the natural small-pox, after the eruption is begun, an instance of which, Mr. Bromfeild himself has given us in his pamphlet, and of which I shall have occasion to speak more at large below. A very judicious and experienced Surgeon of my acquaintance, finding, after he had successfully inoculated a great number, who were previously prepared, that he had very great power over the distemper, began, several months ago, to inoculate without any previous preparation

preparation at all. This gentleman has now inoculated a great many hundreds in this way with the utmost success. And, what is very remarkable, he assures me, he has not used, for many months, a single grain of mercury. He only confines his patients to the usual diet and regimen, and purges them briskly at proper intervals; yet they all have the distemper favourable enough. And another gentleman of the profession informs me, that he likewise has inoculated a small company without using mercury, and that they have all had the distemper very lightly. These relations may serve to shew, that a vast deal, with respect to the success attending the new method of inoculation, depends on the purging used in it, as well as on the diet: and also that the mercury, which is generally administered, is not so *very* powerful an agent in the process, as has been by many imagined. On the whole, I must needs say, I am firmly of opinion, that the abstinence and purging, which are constantly pursued in this method, are the two great causes that render the distemper so extremely light: and that these do more towards effecting this desirable event, than all the other remedies added together. This is my present opinion. I shall, however, be very ready to relinquish it, when I am properly convinced I am wrong.

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But here, methinks, Mr. Bromfeild is ready to ask me, whether these two grand remedies, abstinence and purging, have not been carried sometimes to too great lengths in the new way of inoculation.

As to the first of them, my answer is, I can scarce induce myself to think, that an abstinence of no longer duration, than what is required in inoculation, can ever injure the health of *any* person, who is a proper subject for inoculation at all. As to the other remedy, indeed, the case of that is very different. And I am of opinion, not only that it is very possible for purging in this method to be carried so far, as to injure the patient's health, but also that I have been consulted in some cases, where it has actually been done. But I do not think the same of the abstinence enjoined patients in this method. This is, when continued no longer than is usual in inoculation, a mighty harmless innocent kind of a remedy: and one, whose effects are ever very easily set to rights by a more generous diet. I will, however, venture to pronounce it to be of so very efficacious as well as innocent a nature, that where one person has, through its effect, fallen a sacrifice to the small-pox, either natural or inoculated, at least a thousand have died of that distemper for want of its assistance. It is in no wise my intention in this performance to inquire, how far these remedies

ought to be carried in persons of different ages, habits, and constitutions, under inoculation. I shall therefore observe only on the present occasion, that they are, in a greater or less degree, very necessary in all, in order to insure success to the operation; and that I can inform Mr. Bromfeild of a thing, which he seems scarce to have the least suspicion of, namely, that, unless he pays more regard to them in his future practice, especially to purging after the operation is performed, than he seems to do at present: those *inoculators by the gross*, of whom he speaks rather too contemptuously, will, some of them, far surpass him in success.

I come now to consider a notion of Mr. Bromfeild's, which seems to have taken strong hold of him, and which indeed has, in all probability, contributed more than a little towards leading him into that prejudice, which he has conceived against the new method of inoculating. And this is the following. * *From the universality, he says, of the small-pox, it seems as if nature had some salutary end for the constitution, to be answered by this powerful depuration, that the blood undergoes at such time, which often proves the means of carrying off some other disorders, and afterwards gives a better state of health to the person who has gone through it, I cannot say,*

* Vid. Bromfeild's Thoughts, &c. p. 16.

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I can see any reason there is to suppose, from the nature of the small-pox, or indeed from its consequences, that it was designed by nature as a means of discharging any impurities from the mass of blood, and mending the health of the patient, who catches the distemper. It is true, this may be, and this indeed certainly has, now and then been, the case. But, I am of opinion, whenever it has happened, it has been merely accidental and not owing to any general tendency in the distemper to produce such an effect. Indeed, to say the truth, where it has bettered the health of one person in the natural way, it has sent twenty to their long home. For my own part, I cannot forbear looking on the small-pox in the same light in which most other disorders are to be looked on, namely, that they were designed to be a means of punishing men for their iniquities, of mortifying that pride of heart, to which mankind have ever been too prone, and to bring them to their graves. Nor can I possibly think, that the Almighty had any other end to answer in inflicting this distemper on mankind. Be this as it will, I cannot think, experience shews, that the small-pox is more apt to prove beneficial to the health of mankind, than, for instance, the measles, or the itch, or many other distempers, which are just as universal as that is. If it has now and then proved beneficial in this respect to a patient, it is on the contrary very notorious, that it has now and

then likewise proved highly prejudicial ; and, if I am not much mistaken, this last has been its consequence much oftener, where the pustules have been numerous, and there has been a plentiful suppuration, than where the contrary of these has happened. Mr. Bromfeild, I suppose, would infer from his notion of the salutary effects of the small-pox on the health of patients, who have undergone it, that the new method of inoculation hinders the distemper from being severe enough to answer this his supposed end of nature in exciting it in the natural way. I would, methinks, fain ask Mr. Bromfeild on this occasion, whether or no it be not agreeable to his own experience, that those, who have the small-pox in an extremely favourable manner in the natural way, do usually much sooner, and with fewer obstacles and impediments, recover their former strength and health, than those, who have it in a severe manner. And if this be the case, I would likewise ask him, why patients, who have the small-pox extremely light by inoculation in the new way, may not enjoy their usual health after it, as well as those who have it extremely light in the natural way. I am much mistaken, if it is possible for Mr. Bromfeild, or indeed any other man, to assign one single substantial reason, why they may not, supposing such patients are inoculated by an *experienced and judicious practitioner*. So far as my own experience

rience reaches in matters of this kind, I declare, that those patients, whether they have been inoculated or not, who have had the smallest share of pustules, have ever recovered their pristine health sooner, and have had fewer complaints left afterwards, than those, who have had a large one. As to boils in particular, I have scarce seen one amongst the many patients, in the inoculation of whom I have been concerned. It is true I was consulted for one patient, inoculated by a gentleman at a considerable distance from the place I live in, who by taking cold after the distemper, contracted one of the most malignant ill conditioned abscesses I ever saw, and who escaped very narrowly with his life. But even this case was not owing to a want of eruption, and suppuration of variolous pustules in the patient, since he had a great plenty, which were of the distinct sort, and suppurated kindly. But let us hear, what a judicious writer says of the general consequences of the small pox in the natural way to the health of patients: and by the bye, it is extremely seldom indeed, that there is not both a plentiful eruption, and likewise a plentiful suppuration of pustules in these cases. * *After the distemper is over, he says, there follow inflammations of the eyes, foul ulcers, abscesses, swelling of the joints, pulmonary consumptions, decays and the like. The*

* Vid. Home's Princip. Medicin. P. 185.

truth of the matter is, when the distemper is severe, the variolous venom spreads through, and assimilates as it were, the whole mass of humours in such a manner, that notwithstanding a plentiful eruption, and suppuration of pustules, it is oftentimes a considerable time, before nature is able to correct the universal depravation induced on the fluids by the venom of the distemper. In this respect, the effects of a severe eruption may be, in some measure, compared with those of an exulcerated carcinomatous tumour, the matter formed in which scarce ever fails at length to vitiate the whole mass of humours, and to bring on in the patient, a purulent cacochymy, hectic fever and the like. But what need is there of farther argumentation on the occasion? Since, as the judicious Dr. Baker observes* *one solid practical observation is of more real value, than all the unsupported theories, which the wit of man ever invented.* I appeal to experience, by which all differences in points of opinion with respect to matters of medicine are ever best adjusted. I appeal to practitioners of inoculation, whether they do not find many fewer patients in proportion ill after inoculation in the new way, than they have observed to be ill after it in the old. I ask them in particular, whether patients are so frequently subject to be afflicted with boils in *that* way, as in *this*. I declare, in my *own practice*, I

* Vid. Baker's Inquiry, &c. P. 4.

have scarce seen *one*; much less have I met with a large impostume. Neither do I, at present, know of even a single patient, amongst many hundreds inoculated lately in this way who is not in as good a state of health, as he was before. And I do not doubt, but most other practitioners in the new way have experienced much the same success in this respect, as myself. On the whole, I cannot forbear thinking, that the present method of inoculation is superior to the old. as much in respect of its effects on the health of the patient, as of its exciting a more favourable kind of the distemper.

Here I must beg leave to take notice of another opinion of Mr. Bromfeild's, which he seems to have adopted, in relation to the present practice of inoculation, and which has contributed not a little to aggravate that prejudice, which he has conceived against it. From several passages occurring in his pamphlet, it is evident, he entertains a notion, that in the new method the variolous pustules seldom come to a kindly *maturation*: from whence, I suppose, he would infer, that this method of inoculation is not so safe, with respect to the future health of the patient, as the old. This notion of his is certainly a very erroneous one, since it is very well known, both to practitioners and their patients, that the pustules, when there is any considerable number of them, come to maturation in this way, just as well, as they do in the old, or even

as they do in the natural small-pox itself. And of this he may have ocular demonstration at any time, if he will but give himself the trouble of visiting persons inoculated in this way. It is true, it sometimes happens, that patients under this method pass through the distemper without having even a single pustule, at other times, without having more than two or three. When there are no pustules at all, it is certain no maturation can possibly be expected. But in those cases, wherein there have been two or three only, I have generally observed a little yellow concocted matter on the tops of them, which matter has gradually formed itself into a scab. And it is always possible, even in these cases, were it of any use, to render these pustules larger and fuller of matter, by giving the patients animal food. I have met with many cases of both these kinds in my practice, yet I never could find it was possible to infect the patients again with the small-pox. The reasons, why the eruption and maturation of pustules in these cases are so very inconsiderable, I take to be these. The rigid abstinence and frequent brisk purging pursued in this method, together with the other cooling remedies, which are constantly called in to their assistance, partly so correct, and partly so subtract out of the body of the patient, the bilious and other inflammatory humours, which in the natural disorder afford fuel to the variolous principles contained in the blood, that oftentimes there
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is not pabulum enough left for them to act on. These principles therefore, being put into a state of deflagration by the application of variolous venom, at a time, when there is little or none of this fuel in the blood, burn out, if I may use the expression, quietly, without exciting scarce any commotion or complaint, or more than two or three pustules. And the patient receiving no farther damage, than only being a little weakened by the evacuation and regimen he undergoes, soon recovers his former strength and health. If this opinion be true, we may see plainly, why an eruption and maturation of pustules is oftentimes unnecessary in this way of inoculation, in order to secure the patient either from the contagion of the natural disorder, or from ill health. But what need is there of argumentation on the occasion? *Facts*, as Mr. Bromfeild himself allows, *are stubborn things*. And *facts*, *notorious facts*, abundantly shew, that such cases often happen in the new way of inoculation, and that the patients remain in a good state of health, and are ever after secure from the infection of the natural distemper.

But here, methinks, Mr. Bromfeild is ready to ask me, whether it does not now and then happen in such cases, that on the patients * *returning*

* Vid. Bromfeild's thoughts, &c. P. 30.

to their usual exercise and diet, they have had a second crop of pustules, which have proved to be variolous. My answer is, I have never, that I can remember, met with such a case in my own practice of inoculation. I have, nevertheless, seen two or three such cases, and have been very credibly informed of others. But then I could never find, that the eruption in cases of this sort has been considerable enough to hinder the patients from going about, as in their usual health. The reason of the thing I take to be this. In the natural small-pox the deflagration of the variolous principles in the blood of the patient is performed, if I may so express myself, not only violently, but also suddenly, so that the variolous pustules appear, generally, very early; and it is seldom more than the space of five or six days, from the first invasions of the sickening, before a large eruption is completed. But in patients inoculated in the new way the case seems in these respects to be widely different. And here, by the bye, the new method of inoculation has an inexpressible advantage over the old. In these patients the deflagration of the variolous principles, such are the effects of the remedies used in this method, is performed not only much more quietly, but also more slowly, so that it often continues for a longer time, than in the natural way. Now, as long as this lasteth, so long a propensity remains in the blood, on the application of proper variolous pabulum

pabulum, to throw out pustules. If therefore a patient under these circumstances takes any considerable quantity of animal food, heats himself by violent exercise, or is guilty of the like error, he runs a considerable hazard of getting a second crop of pustules. I make no doubt, but several patients, who have found themselves in pretty good health, whilst this deflagration has been going on very quietly and slowly, and the morbid matter has been running off gently by the pores of the skin, the urinary passages, and other outlets, have through impatience run into irregularities, which have been the means of throwing out fresh pustules. And I am not satisfied, but some patients in this situation, who would scarce ever have had a complaint or a single pustule, have through these irregularities brought on a considerable *eruptive fever* as well as a considerable *eruption*. But I hope Mr. Bromfeild is not so much prejudiced against inoculation in the new way, as to attribute the effects of excesses and irregularities in the patient to a deficiency in the art itself. This would be very unreasonable in him indeed.

But still Mr. Bromfeild, I fear, will be apt to think, that patients in general in this way of inoculation are * *reduced to too low a state*. I really

* Vid. Bromfeild's Thoughts, &c. P. 4.

do not think this has been generally the case. I readily grant, that this circumstance has been now and then carried farther, than has been absolutely necessary; and indeed in here and there a case farther, than has been consistent with a proper regard to the health of the patient. But even in such cases it is in the power of a *circumspect and judicious* practitioner to rectify what may have been done amiss in this respect, by allowing the patients wine, or other cordials, that are stronger, if such are necessary. For as to animal food, I am well satisfied, *that* is never necessary, till the deflagration of the variolous principles is entirely over. I have several times allowed my patients wine and other cordial drinks, when I have thought them rather too low in the sickening; and I never could find in such cases that these remedies rendered the eruption in any wise considerable. But I have ever avoided, as much as I could, giving them any kind of animal food, at this time of the disorder, since I always looked on it, as a nutriment of a suspicious nature in such cases, and much less cardiacal, than wine and other cordial drinks.

Mr. Bromfeild, I find, entertains but a very indifferent opinion of * *brisk purging during the*

* Vid. Bromfeild's Thoughts, &c. P. 30.

eruptive fever in inoculation. I must needs say, I am a little surpris'd at this circumstance, since he himself has given us a most remarkable * instance of the good effects of this remedy in a case of the natural small-pox, which happened in his own family, and in which the remedy was not administered, till after the eruption was begun. For surely, if such purging can do good in this stage of the natural distemper, and in a child too of no more than three years of age, it may be used to advantage in the eruptive fever of adults under the inoculated small-pox. I can assure Mr. Bromfeild, I have purged patients under inoculation during the eruptive fever briskly, both adults and children, fifty times and fifty to that, with advantage to them. I do not, indeed, chuse to do it at this time in *all cases*: because I am satisfied, *all cases* do not require it. And this, Mr. Bromfeild, may see, is very similar to the practice of the judicious † Dr. Dimsdale, a writer on the subject, whom Mr. Bromfeild himself seems much to approve of.

Mr. Bromfeild has observed in one passage ‡ of his pamphlet, that he has *heard*, that *patients inoculated* in the new way, have *afterwards had* the

* Vid. Bromfield's thoughts, P. 20. † Vid. Dimsdale of Inoculating, &c. P. 33. ‡ Vid. Bromfeild's Thoughts, P. 24.

natural

natural small-pox. I have heard the same many a time, and on enquiry have found the reports to be absolutely false. However, it is notorious, that a certain inoculator in this neighbourhood, a *low mechanic*, who had taken it into his head, that it was possible, so far to improve the practice, that the patient might pass through the disorder, without having any complaint, met not long since with such an accident. Four or five of his patients, who were inoculated together in this improved way, took the natural small-pox soon afterwards, and had well nigh paid *extremely* dear for their credulity. And now I am speaking of inoculation being practised by mechanicks, I cannot forbear taking notice of the unaccountable inconstancy and fickleness of temper so very observable in the inhabitants of this isle. Two or three years ago it was a very difficult matter to prevail with many of them, at any rate, to trust their lives in the hands of the most experienced inoculators. Of late, most of them seem to think every illiterate practitioner of this art properly qualified to carry them with safety through the distemper. Strange infatuation indeed! As if it required nothing more, than to order a total abstinence from all animal food, spirituous and fermented liquors, and from spices, to give a few doses of mercurial or antimonial physick, and to make a puncture or two with a lancet infected with variolous matter in the arm, to be able to inoculate with safety to the life

life and future health of a patient. Sure I am, if such illiterate practitioners, as these, to whose care it were almost a piece of madness to trust even a brute, that is sick, are able to practice inoculation even with tolerable success, it amounts to a proof, that the art is arrived to an exceeding high degree of perfection, and that it must of necessity be in the power of those, who have a good knowledge of medicine, to practice it with infinitely more safety to their patients.

But still, I fear, Mr. Bromfeild will not be easily reconciled to one particular part of the treatment used in the new way of inoculating. I mean, the *exposing the patient to external cold, after the eruption is complete*. It is, I must confess, somewhat extraordinary, that Mr. Bromfeild, who seems highly to approve of exposing the patient to cool air, during the eruptive symptoms, as one very likely means of preventing a large eruption, should yet hesitate so much as he does, about the propriety of the same treatment after the eruption is finished. If the maturation of the pustules in the small-pox be a critical operation of nature, so is the eruption of them likewise. And I will be bold to say, there is just as much reason to suppose from theoretical notions, were such notions of any considerable use to determine the question, that external cold is as likely to check their eruption, as it is their maturation. The truth of the matter is,

is, theoretical notions are of little use here. Facts, notorious facts, shew that patients under inoculation may be exposed to the external air during the maturation of the pustules, with as much safety as they can during their eruption, and I cannot forbear adding, however Mr. Bromfeild may be surpris'd at it, with much more safety than they can during a certain other period in the process. I would not willingly be here so understood, as if I supposed, that no mischief could possibly be done by exposing a patient to a cold air during the maturation of the pustules, since I am sufficiently aware, that this part of the treatment in inoculation, may be carried to too great lengths, as well as most of the rest. All I would be understood to mean on this occasion, is, that patients may be safely exposed to the external air, during this stadium of the disorder, as well as during the eruption, provided it be done with prudence and moderation. For here, as well as in all other cases, we are still to keep an eye to the golden maxim of the poet,

*Est modus in rebus, sunt certi denique fines,
Quos ultra citraque nequit consistere rectum.*

I have now, I think, answered every argument and objection Mr. Bromfeild has urged against the new method of inoculation, that is worthy of notice. And, I hope, he has by this time conceived

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at least a more favourable opinion of it, than he entertained, when he wrote his pamphlet. There is, however, one thing which I cannot help, before I have done, desiring he will remember on the occasion. And this is, that I would not have him think, from any thing advanced above, that I look on the present method of inoculation as absolutely infallible, *even* when in the hands of the most *experienced and judicious practitioner*, much less when practised by *ignorant mechanicks*. In the present uncertain and fluctuating state of things where health even in its highest degree is in continual danger from a number of unforeseen accidents, which no human prudence can possibly guard against, and wherein even life itself is not ensured to us for a single moment, it is evident, that a misfortune *may*, nay that it *must*, sometimes happen in inoculation. No practice, no method, whatever can be *altogether* and *absolutely* secure from an accident. Be this as it will, I am very certain there is much less danger of such a misfortune falling out in the new way, than there was in the old. And, should such a thing happen within the circuit of Mr. Bromfeild's knowledge to a judicious practitioner, I hope he will have candour enough to place it to some account, other than that of a deficiency in the present method of inoculating, superior to what must inevitably attend every art concerned in the preservation of so precarious a thing, as human life, confessedly, is.

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I have, I hope, by this time sufficiently convinced both Doctor Langton and Mr. Bromfeild, not only that inoculation in the new way communicates the genuine small pox, but also that it does it in a much safer manner for the patient, than inoculation in the old. I have therefore nothing farther to add on the subject, before I conclude, than an observation or two, which the merits of the present method of inoculating seem highly to demand, namely that I verily believe, if twenty thousand tractable patients were inoculated together in this way by a *judicious practitioner*, not a single one in the whole number would have the confluent small-pox. And I cannot forbear thinking, that as the discovery of the present improved method is an honour to the author of it, whoever that author may be, so it reflects likewise the highest degree of credit upon the art of medicine in general, that it is thus enabled to triumph over a cruel and merciless distemper, which has, for some centuries past, committed greater ravages amongst the human species, than famine, the sword, or even, perhaps the pestilence itself.

ERRATA.

Pro *Bromfield*, l. *Bromfeild*.

P. 6. l. 8. Pro *had been*, l. *had not been*.

P. 40. l. 11. Pro *the crude*, l. *the action of crude*.



F I N I S.